

LETTERS TO THE EDITOR.

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The Thames Valley.

In the "Physical Geology and Geography of Great Britain," Sir A. Ramsay expressed some interesting speculation about the relation formerly existing between the valleys of the Thames and the Severn. According to his view, the Severn Valley was the older, being "one of the oldest in the lowlands of England." He considered that the secondary strata to the south-east of that river originally drained into it, and that subsequent subsidence altered their inclination to an eastward slope, causing the waters to cut a new channel through the Oolites and Chalk towards the east, the direction in which the Thames flows at present. This view, I believe, has never been favourably entertained by other geologists, owing to the absence of corroborative evidence of such a change in the dip of the beds as Ramsay postulated.

Nevertheless, I venture to ask consideration for a feature in the fauna of the Thames Valley which is difficult to reconcile with the belief that the Thames always flowed eastward. I have called it a feature; I should have said, more correctly, the absence of a faunal feature characteristic of other eastward-flowing rivers in England.

In all the rivers between the Yorkshire Ouse and the Norfolk Ouse is found that remarkable fish the burbot, or eel-pout (*Lota vulgaris*), a creature remarkable not only as being the only member of the Gadidae, or cod family, known to inhabit fresh water (the North American *L. maculosa* can be regarded only as a geographical variant of the species), but also on account of its severely restricted distribution in Great Britain. It seems fairly safe to attribute the presence of this fish in the district indicated to the former connection of these rivers—the Trent, the Nen, the two rivers Ouse, &c.—with the great Rhine system at a time when the North Sea was a vast plain, through which these streams found their way to join the mighty river on its course to the Arctic Ocean. The burbot, I believe, abounds in the Rhine at this day; if, as is commonly assumed, the Thames was ever a tributary of the Rhine, why does it contain no burbot?

On the strength of a passage in Leonard Mascall's "Booke of Fishing with Hooke and Line" (1590), I, in common with many others, was led to believe that the burbot did once inhabit the Thames; but I think I can now prove that we have been misled by a printer's or writer's error.

"There is a kind of fish in Holand [not the kingdom of Holland, but the south-eastern district of Lincolnshire] in the fenne beside Peterborough, which they call a poult; they be like in making and greatness to a whiting, but of the colour of the loch [loach]; they come forth of the fenne brookes into the rivers there about, as in Wandsworth river there are many of them. . . . They are taken in welles [eel-baskets] and at waters [weirs] likewise. They are a pleasant meate, and some do thinke they would be as well in other rivers and running waters, as Huntingdon, Ware and such like, if those waters were replenished as they may be with small charge. They have such a plentie in the fenne brookes, they feed their hogges with them. If other rivers were stored with them, it would be good for the commonwealth, as the Carpe which came of late yeares into England. Thus much for the fenne poult."

Now it was easy to suppose that when Mascall wrote of the "Wandsworth river" he meant the Wandle, which joins the Thames at Wandsworth. But if the passage above quoted be read carefully it appears clear that he was treating only of rivers in the fen district, and that he referred, not to Wandsworth on the Thames, but Wansford on the Nen, a few miles west of Peterborough. This explains the difficulty of understanding how a vigorous and prolific fish, once inhabiting the waters of the Thames Valley, and not depending, like the salmon, upon free access to the sea, could have totally disappeared within 300 years. The burbot never inhabited the Thames system, a fact which seems to support Sir A. Ramsay's doctrine that the Thames formed originally part of the Severn

system, with a general flow from east to west, while the basins of the Trent and Yorkshire Ouse were connected with the Rhine system.

HERBERT MAXWELL.

Monreith, Wigtownshire, December 20.

The Inheritance of Mental Characters.

SIR H. BRYAN DONKIN (December 14, p. 210) thinks that I am quibbling, and Dr. Reid (*ibid.*) thinks that I am not clear as to the situation. I cannot argue these points. Suffice it to say that my clearly defined object was and is to show, not that Prof. Pearson's statement quoted by Dr. Reid was right, but that Dr. Reid's condemnation of it was wrong and misleading.

A real difficulty appears to me to lie in the fact that different people use different names for the same thing, and the same name for different things. No so-called character is more than a potentiality in the fertilised ovum. The result of the action of the environment is to produce successive stages in the development of these potentialities. The potentialities, which are subject to variations and may be inherited, are, to me, the only true inborn characters. I gather from Dr. Reid's writings that this is substantially his view.

All the characters quoted from Prof. Pearson are mixtures of acquired and inborn elements. If the fore-arm were never used from birth, it would develop no more than it does in a case of infantile paralysis. If any two children were given precisely the same amount of exercise and of other factors in the environment which influence the development of the potentiality, the development in each would be different. The histories of those remarkable families, the Jukes and Zeros, which produced an enormous number of criminals in a few generations, are well known. Some of the criminal members did not have the same educational environment as their parents. The character dealt with in the fore-arm measurements does not include the presence or absence of the limb. It is development dependent upon a potentiality and a similar, but not identical, environment. Conscientiousness, as dealt with by Prof. Pearson, is development dependent upon the same factors. If it is contended that variations in the environment influence the character, I agree, but in the sense implied here both characters are certainly inherited in the same way.

Dr. Reid again quotes Prof. Pearson, this time as saying that the characters with which he dealt were "bred, not created." I accept Dr. Reid's statement that the meaning implied by "bred" is equivalent to inborn, and by "created" acquired. Having made this quotation, Dr. Reid asks: "Is potentiality meant here?" When I read Prof. Pearson's Huxley lecture I certainly thought that it was. "Geniality and probity and ability may be fostered, indeed . . . but . . . their origin is deeper down than these things. They are bred, not created." Not only this passage, but others, led me to believe that Prof. Pearson, in saying that these characters are inherited, implied that their origins, as distinct from acquirements, are inherited.

Leaving speculations as to Prof. Pearson's private thoughts, and as to how he intended his public statements to be interpreted, may I pursue a more profitable course in asking Dr. Reid for enlightenment as to what *he* means? Dr. Reid's last letter gives me the impression—I am very likely mistaken—that he considers educability, as regards mental characters in man at any rate, as a single potentiality for development, and that the kind of stimulus or stimuli determines which, and to what extent, characters will develop. Now, though I agree with Dr. Reid that individual characters are less certainly inherited than racial, I hold, and I think that he does so too, that the former are the material from which natural selection produces the latter. Unless each of the mental characters is dependent upon a separate potentiality, we must conclude that a change took place whereby all the mental potentialities were massed together; for it is inconceivable that the various adaptive instincts in the lower animals could have evolved otherwise than separately. We are by no means confined to pure instincts or to the lower animals. It would doubtless be possible to teach a bulldog to point, but it is certainly more difficult to teach bulldogs generally to point than it is to teach pointers to point. It is also usually more easy to bring a pointer of good ancestry to a high state of efficiency with regard to his various and

particular mental characters than one of bad ancestry, and a very high state of efficiency is common only in dogs well bred from the Field Trials point of view. These particular mental potentialities are just as much inborn characters as the shape of the dog's head; they are modified by selection in precisely the same way, and are transmitted, with variations, from parents to offspring.

If the existence of heritable variations in each particular mental potentiality be accepted, the conclusion is practically unavoidable that, as Prof. Pearson has suggested, the majority of the lowest class of the population is inferior in capacity for intellectual development to the majority of the middle and upper classes. Favourable variations which are inherited must generally result in a rise, unfavourable in a fall in social position, and a fall to the lowest class means a high mortality among the offspring. Moreover, the upper and middle classes are subjected to continual selection. A known period of selection has changed the Jews from an extraordinarily militant, quarrelsome, and bloodthirsty race to an undoubtedly peaceful, and probably the most generally intellectual race in the world. Any individual amongst them who continued to react to violence by developing a violent temperament must certainly have been eliminated, while survival depended upon a high capacity for making other mental acquirements. The comparative brevity of the period during which selection lasted suggests that mental potentialities respond, if anything, more quickly to selection than do physical potentialities.

Glasgow, December 16.

CHARLES WALKER.

Theory of Complex Cartesian Coordinates.

A RECENT number of the Proceedings of the London Mathematical Society (vol. x., part iii.) contains a note of mine on a theory of complex Cartesian coordinates, in which the complex point $(a+di, b+ei, c+fi)$ is represented by the segment AB joining the real point (a, b, c) to the real point $(a+d, b+e, c+f)$. Since its publication I have learnt that the same theory has been discussed by Mr. Ellery W. Davis in the Nebraska University Studies (Lincoln, 1910).

I am writing now for two purposes. In the first place, I wish to express my regret that, not knowing of Mr. Davis's work, I made no reference to it in my paper. Secondly, the fact that two investigators have quite independently, and both after a study of v. Staudt, invented the same representation of complex points, affords a presumption that it is a natural one. Personally, I believe that many interesting facts will follow from further investigations in this field: it will never replace Staudt's projective theory, which is absolutely perfect so far as it goes, but it may help to make the comprehension of Staudt's work a little easier, and thus popularise one of the most splendid works of mathematical genius.

I may add that parts of Mr. Davis's paper were read before the American Mathematical Society (April and November, 1907; November, 1909) and the British Association (August, 1909). I gave a brief account of the theory myself to the Mathematical Association at a previous date; but really there is no question of priority in dispute. Probably Mr. Davis, like myself, has been in possession of the elements of the theory for a long time.

G. B. MATHEWS.

10 Menai View, Bangor, December 19.

Science and Literary Form.

THE gap between the terminology of commercial science and the ordinary amenities of language seems to be hopelessly widening. The following specimens are culled at random from the account of the exhibition of the Physical Society, contained in *The Times Engineering Supplement* of December 20:—"synchroscope," "decrometer," "lumeter." The word "speedometer" is now consecrated by text-books and even by legislation. All this gives one pause to think, when it is remembered how careful the early scientific pioneers in electrical developments (Kelvin, Maxwell, &c.) were to select suitable terms. The question even persists in obtruding itself, in what relation all this stands to the view that education can be based on a purely scientific training.

J. L.

Cambridge, December 20.

The Weather of 1911.

THE interesting question of Sir Edward Fry in *NATURE* of November 16 can be defined more precisely by the other question: Where did unusual precipitation occur in the

NO. 2200, VOL. 88]

European summer, 1911? I replied to this question, asked by the editor of *Ciel et Terre*, by pointing to the excessive rains of middle and northern China, Japan, and the Philippine Islands in the same summer, 1911. Indeed, the rains of Baguio (Luzon), July 14-17, established a record only comparable with the rains of Cherrapunji, June 12-16, 1874. Baguio received in four days 2239 mm.; Cherrapunji in five days 2598 mm. More comparisons may be found in the *Frankfurter Zeitung*, December 8, ii., and, I hope, at an early date in *Ciel et Terre*. Here I emphasise the connection of these rains, especially in Hondo and Luzon, with typhoons, proved clearly for the latter island in the preliminary communication of P. José Coronas, assistant director of the Manila Observatory: "Three typhoons, which caused heavy floods in Luzon."

In *NATURE* of April 11, 1907, p. 560, I published a paper, "Atmospheric Seesaw-phenomena and the Occurrence of Typhoon Storms." I applied afterwards the law there stated to the weather of the summer of 1907, being on the Atlantic side of the earth extremely cool, in a paper, "Klimaschwankungen und der thermisch-barometrische Ausgleich," in the *Meteorologische Zeitschrift*, 1909, vii., pp. 331-2. Indeed, the formation of tropical cyclones (typhoons) in that summer largely preponderated in the great Pacific focus of such storms. The same explanation as for the cool summer of 1907 can, strange to say, be employed for the dry and hot summer of 1911. A difference is only caused by the larger development of the Azores-maximum of aërial pressure in 1911. This development made the Pacific depressions arriving on the western side of Canada travel more northerly than usual, and therefore arrive in Europe on the more easterly coasts of Russia instead of, as in 1907, on the coasts of western and central Europe. To this also may be ascribed the occasional excesses to night frost in central Europe during June and August, 1911, July descending also to $+2^{\circ}$ C.

The development of the maximum was caused by a somewhat independent and contrary northerly precession of subtropical conditions over Europe and of tropical conditions over subtropical latitudes. These caused another strange phenomenon of aërial pressure in Europe and also in North America, namely, a retrograde motion of depressions similar to the first part of tracks of tropical cyclones, the phenomenon of "Zugstrasse VI.," as I designated it. I found this extremely rare phenomenon over Europe in May, June, and September, 1910, October and November, 1911, and over North America three times in August, 1911. Its occurrence in Europe coincided with well-developed "Hochwassers Tiefs." Conducting them quickly westwards, and preventing them from pouring out plainly their precipitations, it contributed to the relative dryness of October and November, 1911.

WILHELM KREBS.

Grossflottbek (Holstein), December 9.

Nature of Light emitted by Fireflies.

IN *NATURE* of November 23 (vol. lxxxviii., p. 111) there is a letter from Messrs. Singh and Maulik on the nature of the light of the firefly (*Luciola*), in which they report the penetration of opaque substances by the rays from these insects to such an extent as to affect a photographic plate. Their results are essentially similar to those reported by Muraoka (*Wiedemann's Ann. d. Chem. u. Physik*, 1896, vol. ccxcv., pp. 773-81; *Journ. Coll. Sci.*, Tokyo, 1897, vol. ix., pp. 129-39), an explanation of which has been given by Molisch ("Leuchtende Pflanzen," Jena, 1904; Report, Smithsonian Institution, Washington, D.C., 1905, pp. 351-62). The spectral structure of the light of Lampyridæ has been studied spectrophotographically by Ives and Coblentz (*Bulletin of the Bureau of Standards*, Washington, D.C., 1910, vol. vi., pp. 321-36), and also separately by Ives (*Physical Review*, 1910, vol. xxxi., pp. 637-51) and Coblentz (*Physikal. Zeitschrift*, 1911, vol. xii., pp. 917-20; also in *Canad. Entomol.*, 1911, vol. xliii., pp. 355-60).

Before attempting further work along this line Messrs. Singh and Maulik would do well to read the above papers, and to refer to Mangold's monograph "Die Produktion von Licht" (second half, vol. iii., Winterstein's "Handbuch der vergleichende Physiologie," Jena, 1910).

F. ALEX. McDERMOTT.

Industrial Research Laboratory, Pittsburgh, Pa., U.S.A., December 11.